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L'écart entre les valeurs françaises et anglaises subsiste, mais les nombres moyens de B et D sont identiques, une fois l'influence du relai éliminée, tandis que la variation d'équation personnelle de L est de nouveau mise en évidence et fait croître à chaque période les nombres obtenus par H T et L.

Ce sont maintenant les valeurs françaises qui s'accordent entre elles, tandis que les nombres anglais sont discordants. Un tel résultat suffit pour enlever toute confiance aussi bien dans l'une que dans l'autre des valeurs de la longitude. Comme l'a fort bien dit l'astronome royal, il est nécessaire de recommencer la mesure et le plus tôt sera le mieux.

Il faudra, dans une nouvelle détermination, supprimer le relai distributeur, donner à chaque chronographe une pendule séparée, et surtout s'efforcer de s'affranchir des erreurs instrumentales (telles que : mouvements des objectifs, irrégularités des fioles des niveaux et flexions anormales), qui se sont manifestées, tant dans les instruments anglais que dans l'instrument français de Greenwich. Il y aurait, croyons-nous, une garantie sérieuse à échanger les instruments comme les observateurs. Il faut encore noter, comme l'a fait monsieur Turner dans l'Observatory, que les deux seules longitudes faites parallèlement et simultanément par deux couples d'observateurs différents ont donné toutes deux des résultats discordants. Peut-être la précision qu'on est habitué à attribuer aux longitudes télégraphiques n'est elle pas réelle. Les fermetures des polygones jusqu'à présent publiées tendraient à le faire supposer.

On the recent Determination of the Longitude Paris–Greenwich. Reply to Colonel Bassot and Commandant Defforges. By H. H. Turner, M.A., B.Sc.

In my last paper (*Monthly Notices*, vol. li. No. 3, page 155) I pointed out that the independent determinations of the longitude Paris–Greenwich in 1888 by English and French observers did not agree, the original discordance amounting to nearly $0^s{\cdot}4$. I indicated my opinion that the determinations of level-error by the French observers at Greenwich with a striding level were, in some unexplained way, erroneous, and their determinations by reflexion observations from mercury were correct; on which supposition the value obtained for the longitude by the French observers would be in fair agreement with the English result. And I further appealed to the transit-circle observations at Greenwich to confirm this view.

I was aware that these opinions are not shared by the French observers, and should have added a statement of their views had I not hoped that they would make a direct communication on the subject, either in the publications of this Society or elsewhere. And I have now to thank them for the frankness with which they have stated their objections to my reasoning, and for

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the honour they have done me in asking me to communicate the paper to the Society. The longitude Paris–Greenwich is one of such importance, and its determination was undertaken with such care and labour, that it seems to me imperative that any points at issue in connection with it should be clearly stated while yet they are fresh in the minds of those familiar with the work, in order that corresponding precautions may be taken in the future. And accordingly I do not hesitate to give in reply to the foregoing paper the reasons why I personally cannot accept the arguments therein contained.

These arguments are briefly as follow:—

(1) A comparison of personal equations shows that those of the French observers remained satisfactorily constant, while those of the English observers changed. This, as far as it goes, points to a greater accuracy in the French result.

(2) The daily results for time-determination at Greenwich, which I have compared with those obtained with the transit circle, differ so irregularly from the latter as to suggest a systematic error.

(3) Further, this systematic error may be localised in the relay interposed in the clock-circuit at Greenwich in order to convert make-signals into break-signals (to suit the French chronographs kindly lent for the occasion).

(4) Hence this comparison with the transit circle must be rejected, and we have thus no method of choosing between the two discordant results for longitude except that indicated in (1). The work must thus be done over again.

My reply to these paragraphs, in order, is as follows:—

(1) It is perfectly true that the personal equations of the English observers changed, as was very likely to happen in using small instruments for the first time, instead of the transit circle to which they were accustomed. But, owing to the special arrangements made for eliminating the effect of any such change, by a double interchange of observers, the effect on the longitude must be very small—far too small to explain any discordance such as is under consideration. Further, the accordance of the personal equations of the French observers with previous values proves nothing as to the accuracy of the longitude. The personal equations would have come out just as well if they had by some mistake observed a longitude any number of seconds, or minutes, or hours in error; that is to say, a systematic error in time-determination at one end of the line alters the longitude but not the personal equation.

Perhaps, however, the French observers may be referring more particularly to the suggestion in the last paragraph of my former paper—viz. that the error in level-determination to which I attribute the whole discordance was of such a nature as to affect the observations of B differently from those of D; and in this case they quite rightly point to the personal equations as evidence against this supposition. I should like to say one

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or two words in examination of this point. In a letter to me dated 1891 February 5 Commt. Defforges says :—

"Le Colonel Bassot a fait, le 18 Octobre, une série complète d'observations du nadir, qui donnent *pratiquement* le même résultat que les miennes. Voici d'ailleurs les nombres définitifs pour toute la longitude.

β niveau — β nadir.

	s		s
Sept. 30	+0·124	Oct. 6	+0·236
30	+0·121	18	+0·129†
Oct. 1	+0·199	18	+0·120†
2	+0·216	26	+0·269
3	+0·109	26	+0·248
5	+0·125	Nov. 13	+0·157
6	+0·183	13	+0·119

† "Par Bassot: et sans aucune correction aux nombres observés comme vous l'aviez suggéré d'abord."

"Réflexion faite et ayant consulté plusieurs astronomes, nous avons cru devoir prendre, comme vous dites, la moyenne des valeurs données par l'inclinaison et le nadir. Mais vous voyez que la correction appliquée aux niveaux ne peut donc être la cause de la différence que vous signalez entre les longitudes déduites des observations au transit-circle et des observations de Paris."

With respect to the note on October 18, I cannot find on referring to the original paper in which I drew up a list of these discordances any suggested alteration of the "nombres observés" on October 18, but there is a note that the results by striding level [β niveau] for that day were interpolated between those for October 17 and October 19, there being no direct observations on October 18. My data were obtained from the French computation books, the original note-books being in Paris; and no stellar observations having been made on October 18, the striding-level determinations were possibly omitted from these computations, but were recovered on referring to the original records. But I had clearly overlooked the fact that nadirs were taken by Colonel Bassot on this day, for I say in my former paper (p. 163): "It is to be remarked that the systematic correction to nadir observations was applied to the observations of B and D alike, although B did not make any nadir determinations. This may account for the discrepancy between their results." I was thus wrong in saying that B did not make any nadir determinations, but I am not clear that his observations do not rather support my view than condemn it. The mean value of the discordance found by D is $0^{s}{\cdot}176$, and by B (on one day only) is $0^{s}{\cdot}125$. The difference is $0^{s}{\cdot}051$, and its effect on the longitude $0^{s}{\cdot}051 \times \sec. 51^{\circ} = 0^{s}{\cdot}08$, which reduces the discordance shown between B and D on p. 163 of my last paper from $0^{s}{\cdot}20$ to $0^{s}{\cdot}12$. And it is quite possible that the determination on October 18 would be more legitimately compared, not with D's whole series,

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but with the determinations on October 6 and 26, in which case the suggested difference between B and D would be much larger, and would amply explain the whole discordance.

But the simplest way of regarding the matter is to reconstruct Tableau I. of the preceding paper for the two cases:

1. When the observations of the French instrument at Greenwich are reduced with level-errors obtained with the striding level.

2. When they are reduced with level errors given by nadir observations, taking only those nights when nadir observations were made. For the middle series in this case, we have only the single night October 18 on which no stellar observations were made; but I have supposed this nadir determination to apply to October 17 and October 19, as the best we can do.

Tableau I.

	Striding Level.				*Nadir.*			
	H T and L.	B and D.	H T and D.	B and L.	H T and L.	B and D.	H T and D.	B and L.
	s	s	s	s	s	s	s	s
Sept. 30	20·73	20·98	21·23	20·47	20·73	20·79	21·04	20·47
Oct. 1	20·77	21·02	21·25	20·56	20·77	20·71	20·94	20·56
3	20·98	21·12	21·32	20·72	20·98	20·95	21·15	20·72
5	20·97	21·04	21·27	20·73	20·97	20·84	21·07	20·73
Oct. 11	20·65	21·30	20·81	21·18	20·65	...	20·81	...
13	20·69	21·31	20·64	21·37	20·69	...	20·64	...
14	20·72	21·38	20·71	21·39	20·72	...	20·71	...
15	...	...	20·88	...	...	...	20·88	...
17	20·73	21·33	20·84	21·27	20·73	21·14	20·84	21·08
19	20·54	21·31	20·82	21·09	20·54	21·12	20·82	20·90
20	20·60	21·35	20·82	21·11	20·60	...	20·82	...
21	20·62	21·23	20·72	21·15	20·62	...	20·72	...
Oct. 23	21·30	21·10	21·45	20·96	21·30	...	...	20·96
26	21·07	20·97	21·20	20·72	21·07	20·56	20·79	20·72
27	21·19	...	...	...	21·19	...	...	...
30	21·12	21·08	21·36	20·89	21·12	...	...	20·89
Nov. 9	21·18	21·13	21·55	...	21·18	...	...	...
13	21·02	21·97	21·29	20·67	21·02	20·75	21·07	20·67
1st per.	20·86	21·04	21·27	20·62	20·86	20·83	21·05	20·62
2nd „	20·65	21·32	20·78	21·22	20·65	21·13	20·78	20·99
3rd „	21·15	21·05	21·37	20·81	21·15	20·66	20·93	20·81
1 and 2	20·76	21·18	21·02	20·92	20·76	20·98	20·92	20·81
2 and 3	20·90	21·19	21·08	21·02	20·90	20·90	20·86	20·90
Longde.	20·83	21·19	21·06	20·97	20·83	20·94	20·89	20·86

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The results for longitude in the second case are thus moderately accordant, while those in the first are discordant. The accidental errors in the second case are of course larger, as the level-error depends on a single determination on each night instead of several.

The personal equations are as follow in the second case :—

HT–L	B–D	HT–D	B–L
s	s	s	s
−0·10	+0·15	−0·14	+0·19
−0·25	+0·24	−0·08	+0·09
−0·18	+0·19	−0·11	+0·14

in which there is nothing extravagantly discordant.

I do not insist that in view of the above systematic discrepancy between nadir and striding-level determinations one or other is clearly wrong; but it does seem to me that so large a systematic difference *suggests* some instrumental defect. The difference is larger than accidental differences should be. Did the observations stand alone, the only course would be to take the mean of the two series, as has been done in the French reductions. But the independent English operations seem to me to supply just the additional evidence required to enable us to choose between the two alternatives, instead of taking the mean.

(2) The differences of time-determination by the transit circle and by the longitude instruments are certainly somewhat irregular, but quite within the limits of what is possible, considering that different instruments and observers are compared. The parallelism of the four curves is not more pronounced than it should be, considering that one instrument (the transit circle) is compared with each of four others; and consequently its errors are common to the four curves. The obvious dip on October 26, for instance, is undoubtedly due to the transit-circle observations; and on careful scrutiny can be traced to a bad observation of *Polaris* by a young observer, from which the azimuth error on this day is deduced. It is to be remarked that the errors of the transit circle, though attributed to the observer A D, are really determined by many different observers, and reduced to A D by applying the assumed differences of personal equation.

(3) We now come to the real point at issue—the constancy of action of the relay. It is suggested that this time of action varied to the extent of some tenths of a second, according to the adjustment of the armature! If this were possible, it would make it almost impracticable to use relays in such work; but, fortunately, all the direct evidence available is against such an assumption.

In the first place, the indications of the sidereal standard through the relay on the longitude chronographs (E and F) were on many days directly compared with those without relay

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on the Royal Observatory chronograph (R O) used in transit-circle observations. The following are the results :—

Date.	RO−E	RO−F
	s	s
Sept. 29	+0·04	+0·05
Oct. 17	+0·08	+0·08
19	+0·06	+0·05
20	+0·02	...
21	+0·03	...
23*	−0·02	...
27	+0·05	...
Nov. 13	+0·09	...

* On October 23 there is some evidence that one of the pens of chronograph E was loose.

These differences are not corrected for the "parallax of prickers" of R O. This quantity is about 0s·04, but is neglected as constant in all transit-circle observations. Its sign is such as to diminish the above positive quantities, so that the time of action of the relay was always very small indeed. It is perhaps unfortunate that these comparisons were not made oftener, but it will be noticed that they were made in all three periods, and indicate no sensible difference in adjustment in the three periods, although the relay was adjusted by D or L in the first and third periods, and by B or H T in the middle period.

In the second place, if there were any fault in the relay, both French and English observations at Greenwich should be affected similarly. But nothing is clearer than that the fault, whatever it may be, affects the observations in opposite directions, or affects one and not the other. The difference between the time-determinations by B and H T, which was about − 0s·28 at Paris during the first period, became − 0s·43 at Greenwich during the second if the striding-level determination be accepted and returned to − 0s·32 at Paris during the third. Similarly, the difference D − L was − 0s·30 at Greenwich, became + 0s·08 at Paris, and returned to − 0s·12 at Greenwich.

If, then, we assume the Paris observations (against which nothing has been alleged by anybody) to be correct, the personal equations B − H T and D − L should be − 0s·30 and + 0s·08. But at Greenwich, if we accept the observations of striding level, these are found to be − 0s·43 and − 0s·21 respectively; and the action of the relay is here eliminated, for it was common to B and H T, and to D and L in the two cases. On the other hand, the nadir determinations, as far as they go, give − 0s·35 and + 0s·14.

For these two reasons, (*a*) that the time of action of the relay was, on all occasions on which it was tested, found to be

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extremely small; (β) that its effect on time-determination was common to both observers at Greenwich, it does not seem to me that the discordance we are considering can be put down to the relay.

(4) The fourth paragraph in which I have summed up the paper of the French observers deals rather with matters of opinion. I have endeavoured in what precedes to keep mainly to facts. But, perhaps, I may say briefly that there does not seem to me to be any reason for rejecting the comparison with transit-circle observations, which was proposed by the Astronomer-Royal as an important item in the programme when the plan of the work was being discussed, and is a most valuable check on the results. Further, no clear reason has been given why the English result, $9^{\mathrm{m}}\ 20^{\mathrm{s}}{\cdot}86$, should not be near the truth (for the admitted variation in personality cannot seriously affect the longitude), and there is a very simple supposition, as has been pointed out in the earlier part of this paper, which would bring the French result into accordance with this.

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Observations of Phenomena of Jupiter's Satellites at Windsor, New South Wales, in the Year 1890. By John Tebbutt.

Day of Obs.	Satellite.	Phenomenon.	Phase.	Aperture of Telescope.	Mag. Power.	Greenwich Mean Time of Observation. h m s	Mean Time of Nautical Almanac. h m s
July 14	I.	Tr. Ingr.	Ext. contact	8-inch	230	0 17 25	0 19
14	I.	,,	Bisection	,,	,,	0 19 5	
14	I.	,,	Int. contact	,,	,,	0 21 5	
14	I.	Occ. R.	First seen	,,	140	23 54 20	23 56
14	I.	,,	Bisection	,,	,,	23 55 24	
14	I.	,,	Last contact	,,	,,	23 56 59	
21	I.	Ecl. D.	Last seen	,,	130	23 9 55	23 9 57
22	I.	Tr. Egr.	Int. contact	,,	110	22 42 36	22 49
22	I.	,,	Bisection	,,	,,	22 45 46	
22	I.	,,	Ext. contact	,,	,,	22 48 6	
31	II.	Occ. D.	First contact	,,	130	21 46 22	21 49
31	II.	,,	Bisection	,,	,,	21 49 26	
Aug. 13	IV.	Tr. Egr.	Bisection	,,	110	20 17 52	20 26
13	IV.	,,	Ext. contact	,,	,,	20 21 41	
13	I.	Occ. D.	First contact	,,	170	22 54 50	22 58
13	I.	,,	Bisection	,,	,,	22 58 15	
13	I.	,,	Last seen	,,	,,	23 0 4	
14	I.	Tr. Ingr.	Ext. contact	,,	130	20 5 52	20 6
14	I.	,,	Bisection	,,	,,	20 7 42	
14	I.	,,	Int. contact	,,	,,	20 10 37	